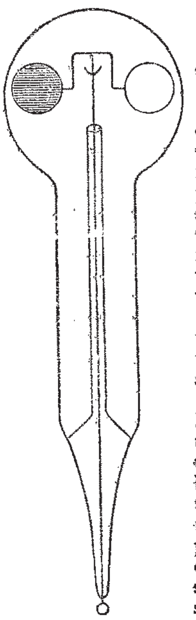
Action on Frog's Eye of Spectrum of Oxyhydrogen Flame.

	Initial Effect.	Final Effect.
Yellow, near orange	rise of 70	rise of 10
Green yellow	,, 25	,, 5
Green—low	,, 15	,, 0
Green—high	,, 15	,, 0
Green—higher	,, 18	,, 8
Yellow green	,, 85	,, 35
Yellow	,, 80	,, 40

(*To be continued.*)

SIR WILLIAM GROVE ON THE RADIOMETER[1]

SIR WILLIAM GROVE described some experiments he had recently made with a modification of Crookes's radiometer. After a few prefatory trials, such as coating one-half of the bulb with tinfoil and electrising it, which gave no notable results, he devised a method, shown in the accompanying sketch, by which he could electrise the whole of the internal system. Four aluminium vanes, each blackened on one side, had metallic arms and a metal point at their crossing that rested in a metal cup. The latter was united to a platinum wire that passed through a glass tube and was fused into it, the platinum wire protruding. Lastly, the glass tube was fused inside the apparatus and hermetically sealed, the end of the platinum wire being exposed. The vacuum in this apparatus was considered by Mr. Crookes[2] to be as perfect as in his radiometers generally, but Sir Willam Grove doubted that it was so. The following were the results:—



1. With the faint light of a lucifer match or of one or two candles, the vanes invariably turned the opposite way to the normal, the polished surface being repelled. With a dark heat, as from an iron shovel heated short of redness, they went the normal way. These effects continued for several days, but not permanently; the apparatus seemed to have leaked and to have become sluggish and irregular.

2. On electrising the protruding platinum wire with a rubbed rod of glass or sealing-wax, the vanes rotated sometimes one way and sometimes the other.

3. On connecting the negative pole of a Ruhmkorf's coil the results were uncertain, but the positive pole caused the vanes to rotate steadily, and its effect was even better than that from light or heat. In the dark the effect was very beautiful, as the dark vanes moved through a phosphorescent glow. The total results were considered by Sir William Grove to be somewhat negative, but they tended to show that all the effects were due to residual air. He suggested in explanation of the last experiment, that more electricity would escape from the rough than from the polished faces of the vanes, as the former presented a vast number of points. Consequently the rough faces would produce more disturbance of the gas in front of them, and would themselves be more affected by the reaction than the plane faces. The polished surfaces being repelled by luminous heat is, however, very difficult of explanation.

In his second notice Sir William Grove described some further experiments he had made with Crookes's radiometers since the last meeting of the club. He did not now entertain much doubt that these movements are due to the effect of residual air. Mr. Crookes had kindly made a second instrument for him, and the one that he described at the last meeting, of which the vanes were metallic and in metallic connection with a platinum wire that protruded outside the apparatus, had been re-exhausted. Both now act normally, the black faces of the vanes being repelled by light and by heat. When the protruding wire is now electrised by a Ruhmkorf's coil the effects that were previously observed are altogether absent, there is not the slightest luminosity round the vanes, and the current does not pass. But although the current is now incapable of traversing the small space of one-tenth of an inch that separates the vanes from the glass, induction acts across it just as well as before. This is shown by the readiness with which the vanes follow the movements of a piece of rubbed glass or sealing-wax held near the apparatus. It is therefore evident that the effects of attenuation of air upon discharge and upon induction are not the same. When attenuation has commenced and is increasing, the discharge passes more and more rapidly, until it becomes a glow, or according to the old theory of electricity, polarisation becomes more and more readily subverted; but a further attenuation stops the discharge entirely. On the other hand, induction continues, and appears to be in no way lessened by extreme attenuation. These results cannot be accounted for by the old theory that discharge is the consequence of subverted induction.

It farther appears that a radiometer is a most delicate electroscope. By tilting it until the vanes touch the glass, the interior of the glass may be electrised, and it will then remain for days in that condition. He had performed this operation eight days ago, and the movements of the instrument by light or heat have been thereby wholly checked. Every endeavour has been made to discharge or neutralise the electricity on the glass surface, as, for example, by covering the exterior of the globe with tinfoil and connecting this with the platinum wire, nevertheless the glass remains charged, showing what a perfect insulator a good vacuum is.[1]

The above is a copy of the abstracts in the club book. They are now further published, as some partial notices of them have appeared in foreign journals. W. R. G.

THE NORWEGIAN NORTH SEA EXPEDITION, 1876[2]

II.

Researches relating to the Salt-water Fisheries.

BY the side of the more strictly scientific researches it was also our intention during the expedition, if opportunity offered, to give close attention to all the circumstances that might stand in any connection with or throw any light upon our most important salt-water fisheries. As I already during a series of years had been engaged in the study of our fisheries, the prosecution of these researches was committed to me.

For this reason there was added to our other equipment various fishing apparatus, as hooks and lines for deep-sea fishing, and several sorts of drag-nets with various sizes of mesh. The use of such implements could, as a matter of course, only be reckoned upon in good weather and with a pretty smooth sea, which we, however, had promised ourselves might occur at least now and then during our three months' excursion at the best season of the year. But the state of the weather was unfortunately so utterly unfavourable during our whole expedition that the employment of the apparatus we have referred to was not to be thought of. For the same reason the apparatus for measuring the velocity of the currents, exceedingly important in the first place for the physico-meteorological researches, but also for those with which we are now concerned, could not be brought into use. During the few fine days we had in the course of our expedition we were too near the coast for these researches to have any special interest.

Although the state of the weather thus laid insurmountable obstacles in the way of the researches referred to, I have, however, during our expedition, been able to establish certain facts which, in my opinion, are of no inconsiderable importance in this direction, and will be of great use in guiding us in the continued practical scientific researches concerning our fisheries. It is of these facts that I now proceed to give some details.

It is ascertained by our soundings that off our coast there are several fish-banks of whose existence there was no previous knowledge, and on which a profitable fishery with bank vessels may certainly be carried on during the summer months.

The so-called "Storegg" (great edge) off Romsdal's Amt has been from old times famous for its immeasurable richness in fish, and there has been an obscure tradition that it was not the only point where such fishing could be carried on on a large scale, but that there were to be found similar rich fish banks at many other points far out in the open sea, "were man only fortunate enough to fall upon them." The mystic account of the "Havbro" (sea pier or jetty) has now been for the most part explained by the surveys set on foot during our expedition. The "Storegg" is nothing else than a piece of the edge of the extended barrier, which in the west forms the boundary line beyond which lie the cold polar sea-deeps. That so clear a knowledge of this edge within a very limited extent has been already obtained, without the least idea being formed as to its proper connection, naturally arises from the polar sea-deeps here running in closer to the coast than at any other point. A new piece of the continuation of this edge had already been found by soundings made from the steamer *Hansteen*, and we have now been able to establish its existence at several other points, and that both farther south and farther north it retires more and more from the coast to a distance of from twenty to thirty Norwegian (140 to 210 English) miles.

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[1] Abstract of two communications by the Hon. Sir William Grove, F.R.S., to the Philosophical Club, May 18 and June 15, 1876.

[2] Who kindly made it for me from my description. —W. R. G.

[1] I may state that the electricity did ultimately become dissipated, but not until several weeks had elapsed.—W. R. G.

[2] By Prof. G. D. Sars From Christiania *Dagbladet* of January 27. Continued from p 414.

Although it may not be constant everywhere, it appears, however, to be the rule that at the bottom, at the farthest boundary of the barrier, before it slopes towards the great depths lying beyond it, it rises somewhat, and assumes simultaneously a hard stony character, as is the case, as is well known, at the Storegg. At the first sounding, when we went out from Husoe, we struck this edge at about twenty Norwegian (140 English) miles' distance from the coast (stations 16 [1] and 17 [2]). The bottom, which before had everywhere appeared to be soft, suddenly, at a depth of 221 fathoms, became hard and stony, and retained this character even after it had sloped about fifty fathoms down towards the deep sea lying beyond. That there was here a pretty abrupt descent is clear from the circumstance that we already at the next station reached far down into the cold area with a depth of 412 fathoms, and a bottom temperature of ÷ 1°·3 C. Farther north, about the latitude of Trondhjem, we found at a depth of 190 fathoms, and likewise on the boundary line between the warm and cold area, a very similar edge with rocky bottom, which falls off with a pretty steep slope towards the west (Station 89 [3]). Also on the opposite side of the tract of sea we traversed, we had occasion to observe a similar state of things. Off the Færoe Islands, and at a considerable distance from them, we were fortunate enough, though the weather was exceedingly unfavourable, to find the outer edge or opposite point of the extensive Færoe bank (Station 38 [4]), whereby its extent and configuration could be to some extent determined, and the state of things here appears to be very similar to the Storegg.

By the carefully-planned soundings which were undertaken from Ramsen Fiord westwards, there could be established at a comparatively inconsiderable distance from the coast, the existence of a hitherto quite unknown, well-defined, steep bank of considerable extent, with a hard bottom, and a depth of only 62–93 fathoms (Stations 63,[5] 64,[6] 65[7]). Beyond this there was a very gentle and even descent towards the great deeps, but we did not here meet with any true edge as at the Storegg. It may be added that in the outer part of Søgne Fiord (Sognfjœen), we found a pretty extensive plateau, with a hard stony bottom, and a slope both inwards and outwards (depth from 206 to 211 fathoms).

That all the points mentioned above are excellent fishing-grounds I have not a moment's doubt. Everywhere, where at a considerable distance from the coast, such banks with hard or stony bottom have been found, there have always on closer examination been found large quantities of fish, and although an attempt made by us by attaching to the lead a short line with hooks and bait was unsuccessful, there cannot be any negative conclusion drawn from this method of research, which was unfortunately, by reason of circumstances, very unsuitable for the purpose.

The kinds of fish which are found on the sea-banks are, as is well known, principally ling, torsk (*Brosmus vulgaris*), halibut, and cod, the so-called bank cod. I have already, in my reports to the department, clearly set it forth as my opinion that the so-called bank cod is not a different variety from the well-known winter cod, or *skreid*, which in winter and all through the spring, visits our coast for the purpose of spawning. The earlier hypotheses concerning the migrations of the winter cod (*skreid*) from great distances in the sea, I have, after a close study of the nature of this fish, been obliged entirely to abandon, and the experience obtained during our expedition confirms me in this. It is my conviction that the winter cod, which is to be found along our coasts during winter, and which is the object of some of our most important fisheries, is during the rest of the year distributed only over that tract of sea whose bottom forms the barrier against the polar sea-deeps lying beyond it, and that the outer boundary of this barrier (the so-called Havbro), with its well-developed animal life and favourable bottom, forms a suitable habitat for innumerable multitudes of this fish.

Very dissimilar are the circumstances with reference to the second of the varieties of fish most important for our fisheries—the herring. Here my earlier researches have led me just to the opposite conclusion. While the cod is evidently a genuine bottom fish, and as such dependent on the nature of the bottom and partly on the depth, the herring, on the contrary, in consequence of its whole nature, is a genuine pelagian fish, and its occurrence is therefore exceedingly independent of the depth or the nature of the bottom, but, on the contrary, dependent on the physical and biological conditions in the upper stratum of the sea. As these are very changeable, this species of fish may have been furnished with means to enable it speedily to seek out the most favourable tract of sea. The herring has also, as contrasted with the cod, obtained its elegantly-compressed, wedge-like form, whereby with the speed of an arrow it can shoot along through the water, and in a comparatively short time traverse long distances. Although I do not adopt the old ideas, according to which the spring herring comes as it were from the ice-covered sea about the North Pole, I am, however, inclined to believe that, not only when it visits the coast to spawn, but also during the rest of the year, it undertakes irregular migrations in the open sea. The distribution of the herring in the sea is dependent on the distribution of the small animals which form its food. These small animals are all pelagian, mainly small crustacea of the order Copepoda, which keep more or less near the surface of the sea, and are commonly known by our fishermen under the name of "aat." Only when the herring during winter resorts to the coast to deposit its spawn are its movements for the time independent of the occurrence of "aat." The whole other part of the year, on the contrary, the shoals range through the open sea, inasmuch as they prefer to betake themselves to that region of the sea where, at various seasons, there is the greatest abundance of "aat." The great mass of the herring shoals can thus very naturally, towards the approach of winter, or at the time when the development of the organs of generation drives them to resort to the coast in order to spawn, be found sometimes at a less, sometimes at a greater distance from their spawning places according as the sea in one direction or another has the greatest abundance in "aat." On this again mainly depends, I am convinced, the fluctuations in our spring herring fisheries. For as the spawning migration begins long before the roe or milt are ready to be deposited, the mass of herring, if at that point of time it finds itself close to the coast, will reach it so early that it will be obliged to remain there for a considerable time, during which it will naturally come to seek closer in towards the coast in the fiords and bays. In the contrary case, when the mass of herring at this point of time finds itself at a considerable distance from its spawning places, so long time will have passed before it reaches them that the spawning process will go on immediately after their arrival at the coast. The herring will then remain only a short time along the coast, and the spawning will then for the most part be carried on on the outermost banks, less accessible to the fishermen, in other words, the spring herring fishery will be very short or exceedingly unsuccessful.

This is, in short, the theory which I already, several years ago, in consequence of researches made by me along our coasts, was led to advance as in my view the only probable scientific explanation of the remarkable irregularities which in course of time are observable in our spring herring fisheries. I have, however, unfortunately this time only very few facts to support my theory with, and I cannot, therefore, be surprised if it has been received with mistrust, as merely a hypothesis. There are, indeed, a few reports from seamen of their having observed large herring shoals far out in the open sea, immediately before the beginning of the spring herring fishing, as there have been observed by others at various seasons of the year great masses of "aat" at different points in the sea, and we have information concerning this last phenomenon, partly also from trustworthy scientific men (Kroeyer), and that just from that region of the sea, which here most interests us; but these statements were, however, too few to form complete evidence that the open sea is in fact a suitable dwelling-place for the enormous masses of herring which at certain seasons of the year move towards the coast.

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[1] Station 16, lat. 62° 23′ 9″; long. 2° 17′ E. from Greenwich.
[2] Station 17, lat. 62° 33′; long. 2° 4′ E. from Greenwich.
[3] Station 89, lat. 64° 1′; long. 6° 7′ 5″ E. from Greenwich.
[4] Station 38, lat. 62° 57′; long. 3° 47′ W. from Greenwich.
[5] Station 63, 64° 41′ 3″ N. L., 9° E. from Greenwich.
[6] Station 64, 64° 42′ N. L., 8° 50′ E. from Greenwich.
[7] Station 65, 64° 42′ 5″ N. L., 8° 39′ E. from Greenwich.

During our expedition I therefore considered it a very important object to examine closely the distribution of the "aat" in the tract of sea over which we sailed. For this purpose the sea was examined almost daily, often several times a day, by the help of a surface-net. The results of these examinations completely confirmed my previous view on this point. During the whole passage from Norway to the Færoe Islands, the sea was found everywhere filled with enormous masses of the so-called "red aat" (almost exclusively *Calanus finmarchicus*) which, as is well known, forms the food best liked by the herring, and, what deserves to be remarked, the quantity of this "aat" appears to increase with the distance from the coast, being greatest at a distance of about twenty Norwegian (140 English) miles. Besides the "red aat" we also observed farther out to sea, great quantities of another pretty blue sort of "aat" (*Pontella Patersonii*), which appears to belong more to the Atlantic Ocean, and which, to distinguish it from the other, might be called the "mackerel aat," as it probably forms the principal food of the mackerel at those seasons of the year when this fish is not in the neighbourhood of the coast. This "aat" also shows itself sometimes, particularly during great takes of herring in summer, among the "red aat" close to the coast. When we went northwards from the Færoe Islands toward Iceland, it was remarkable that the "aat" almost entirely disappeared from the sea. At the same time the sea had assumed a very different colour. While during the whole passage from Norway to the Færoe Islands it had been a deep blue, it was now a light, dirty, greyish-green. This peculiar circumstance, for which I cannot yet account, but in which a peculiar relation of the ocean currents certainly plays a considerable part, appears to stand in close connection with the occurrence of "aat," and will be the subject of careful researches during our next expedition. I had a very convenient opportunity of observing this phenomenon from my cabin, the light of which was almost on a level with the sea. When, by the pitching of the vessel, the glass was washed over, the whole cabin was clearly illuminated with a very beautiful, intense dark blue light, and I have often, when, after my work was ended, I was taking a little rest in my cabin, been greatly delighted with this phenomenon, which so strikingly reminded me of my stay in the south the preceding winter, and my ever-memorable visit to the blue grotto at Capri. Now, on the contrary, the illumination was quite different, namely, light greenish. This colour remained constant so long as we were in the navigable water near Iceland, and the sea was everywhere, as has been stated, almost completely free from "aat." The previously-observed state of things recurred first when we, on our return voyage, approached the coast of Norway. The water resumed its beautiful blue colour, and the sea swarmed with "aat." I cannot help supposing that the conditions observed during our expedition is not always the same, as several recent accounts state that the sea about Iceland is specially rich in "aat." It appears as if the constant westerly storm, which we had to put up with during our expedition, in combination with the strong up-going current, had had a disturbing action, and forced the mass of "aat" farther in towards the Norwegian coast. If this should in fact be the case, a supposition which in the mean time with the little experience we have yet had on the point can scarcely be supported with full evidence, there may be seen in this (if the above-mentioned theory of mine with reference to the migrations of the herring be accepted) a good omen of the improvement of the spring herring fishery in the near future. That the herring is where the herring food (the "aat") is, I consider a settled point. Although we unfortunately had no opportunity of directly establishing the presence of herring by the help of our nets, there were not wanting the best signs of it at the points where the "aat" was most numerous. Not a few whales (both *sildehval*, *Physalus antiquorum*, and *staurhyrning*, *Orca gladiator*) were observed at such places, as well as large numbers of birds (chiefly kittiwakes), and, at a considerable distance from the coast at stations 75 [1] and 76 [2], there were large brown spots in the sea, like extensive sea-weed fields, but which on a closer examination were found to be enormous masses of "aat" closely packed together, on which the fulmar petrels (*procellaria glacialis*), our constant companions during our excursion, feasted to their heart's delight. That these enormous "aat" masses could not be packed together here by pure accident is evident, and that the current alone should be able to do this here far out in the open sea I cannot believe. I am rather of opinion that the herring shoals have driven this "aat," together in the same way as may often be observed in the case of coal-fish, and that there, under these brown spots on the sea, there were enormous shoals of herring (*sildebjerge*).

I am much disappointed that circumstances did not permit us to use our nets here. We might have been able in this way readily to establish the occurrence of the herring far out at sea. It is to be hoped in the mean time that in our next expedition we shall be more fortunate in the weather, and we shall then put this herring question in the first rank, the rather as we shall be then farther north or nearer the waters, which, in my opinion, are the proper home of spring herrings (*vaarsilden*) and the great herrings (*storsildens*).

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[1] Station 75, lat. 64° 47′ 2″; long. 7° 13′ E. from Greenwich.
[2] Station 76, lat. 64° 47′ 4″; long. 7° 3′ 6″ E. from Greenwich.

OUR ASTRONOMICAL COLUMN

THE SUSPECTED INTRA-MERCURIAL PLANET.—M. Leverrier, in a circular addressed to astronomers, has again directed attention to the importance of close and frequent observations of the sun's disc, on March 21, 22, and 23, but especially on the intermediate date, with the view to detect the small planet, which he assumes to have been already observed in transit on six occasions, and which there would appear to be just a possibility, may be again projected upon the face of the sun at this time. In his reasoning upon this subject, M. Leverrier adopts for the place of the node, the value he had deduced from the well-known observations of Dr. Lescarbault on March 26, 1859, but the uncertainty attaching to the result renders it impossible to pronounce definitively on the occurrence of a transit in the present month.

The six observations to which reference is made above are those of Fritsch, at Quedlinburg, October 10, 1802; Stark, at Augsburg, October 9, 1819; Decuppis, at Rome, October 2, 1839; Sidebotham, at Manchester, March 12, 1849; Lescarbault, at Orgères, March 26, 1859; Lummis, at Manchester, March 20, 1862.

Attributing these observations to the passage of a single planet across the sun's disc, he found a formula for the heliocentric longitude at any time, in which an indeterminate entered, allowing of several solutions of the problem of finding the period of revolution, and hence the mean distance of the body from the sun. Two of the solutions appear to possess equal precision in the representation of the observations; in the first, the time of revolution is found to be 33·02 days, and the mean distance from the sun 0·201, that of the earth being taken as unity: in the second solution the length of the revolution is 27·96 days, and the mean distance 0·180. Whichever period we adopt, we find from M. Leverrier's formula that the suspected planet should be in conjunction with the sun on March 22, astronomical reckoning, for the meridian of Greenwich, though to decide definitively as to the passage or otherwise of a planet across the sun's disc at this time, it will be necessary to examine it not only throughout the whole of the corresponding revolution of the earth upon her axis, but owing to uncertainty in the data for prediction, during the twenty-four hours preceding and following, or as already stated, on March 21, 22, and 23.

It is difficult to understand how six observers, without, as M. Leverrier remarks, any relation with each other, nor any knowledge of the periods under discussion, can have fallen by chance upon six exact epochs of a phenomenon explicable by the motion of a single planet. Though suspicion has attached in the minds of some astronomers to one or two of the observations to which we have referred, the fact pointed out by the illustrious French astronomer does appear very strongly confirmative of their reality. At any rate, the existence or otherwise of such a body may be decided by systematic examination of the sun's disc, near the calculated epochs of conjunction, within the assumed transit-limits; but it so happens that after the present month there is very little probability of a transit